V. CORRELATION OF SEDIMENTARY AND CLIMATIC RECORDS

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Hitchcock as early as 1841 suggested that the clays of the Connecticut Valley showed annual banding. Certain regular alternations in Cretaceous strata in Colorado were thought by G. K. Gilbert (1895) to have a possible correlation with the precessional period of about 21,000 years; and by using this period as a time unit he calculated that 3900 feet of sediments (Benton, Niobrara and Pierre) were deposited in 20,000,000 years. Berkey (1905) provisionally interpreted the double (winter and summer) laminations in a glacial clay bed 35 feet thick at Grantsburg, Wisconsin, as a measure of the years, 1700 in all, in which the deposit was laid down.

It will be observed that Gilbert and Berkey made studies in a single location. They were seeking measures of the periods of time in which given deposits were laid down—measures akin to those of Michelson on the speed of light, in that the end in view was a more accurate yardstick. The interpretation of sediments has now passed into two more advanced stages: (1) the cross-dating of sediments laid down in separate localities under conditions so nearly comparable as to provide, not identical, but at least closely similar records, and (2) tentative correlations between such equivalent records and other records of similar or greater precision such as the tree ring analyses of Douglass and the solar radiation results of Abbot.

De Geer's work on the "varved" clays of Sweden and Antevs' critical field studies in New England and eastern Canada are among the most noteworthy of the attempts to cross-date sediments. De Geer, looking for a general geochronological scale, boldly seeks to correlate the "curves" of banded clays (varves) from Asia, South America and North America with the varve curves of Sweden, while Antevs challenges these results and insists on a more rigid theoretical basis and on much wider field work, namely, the checking of such correlations by the study of the record of contemporaneous or at least parallel events in moraines, eskers, striae and the like. A number of observers have suggested the possible correlation of the varying thickness of varved clay bands, or ups-and-downs in varve "curves," with changes in solar radiation; and Reeds[1] has essayed such a comparison. The theory runs as follows: changes in the degree of solar radiation affect the melt rate of ice through variations of rainfall, temperature and cloudiness. The yearly layers of transported sediment—each composed of a thicker and lighter summer layer and a thinner and darker winter layer—are reasonably assumed to vary in thickness accordingly.

The so-called varved clays are of glacial-marginal origin. The lakes in which they were deposited were provided with sediment-bearing water that flowed from the front of the continental ice sheet. A great deal of rock flour—fine silt and clay—was included in the discharge. While the coarser detritus came to rest near shore the fine silt was widely distributed over the lake floors before it came to rest and the clay remained in suspension for a much longer period and upon deposition formed the characteristic dark winter band. The strongly marked banding in clays of glacial-marginal lakes reflects the strong contrasts of rigid winter and warmth-compelled summer discharge on the front of a retreating ice border.

The fresh-water lakes in question were short-lived. With the retreat of the ice that impounded them, outlets were provided at lower levels and, in many cases, the entire lake floor was laid bare. Within distances of a few miles up to 100 or 200 miles such lakes may have existed in the same period or portions of the same period and thus have had corresponding experiences which have been found to be clearly identifiable. The rate of retreat of the ice—about a mile in 22 years in central New England—the extent of the common experiences which given lakes enjoyed, and even the record of exceptional catastrophic events in the life of a given lake, such as a change of outlet, are among the results of varve studies. All such datings are relative. No one has yet found the bottom of the oldest deposit nor the top of the youngest.

When we come to the correlation of the varved clays with tree rings there is an obvious gap between even the oldest New England trees and the date of extinction of the much more remote lakes in which there were formed seasonally banded clays. One naturally turns to the West, where trees grow that are more than 3000 years old. The most promising localities in the Great Basin have been searched for varves and a few sequences have been identified by Antevs[2]; but they are short and he concludes: "Correlation between rate of growth of the Big Tree and fluctuations of the lakes in the arid regions in the western States should be avoided, until the conditions governing the growth of the sequoia are better known, and the datings of the lake fluctuations are more accurate."

There are two prime difficulties in a tree-lake correlation in the Great Basin. First, the Great Basin lakes are and have been mostly salt, with short fresh-water interruptions; and the salt flocculates the sediment so that it is deposited promptly, leaving little colloidal material to settle upon the off-shore reaches of the lake floor. Second, there is a gap between the complicated history of the Great Basin lakes and the oldest trees. Two steps seem indicated. The late sedimentary history of the Great Basin requires much more detailed study by modern techniques before we have a satisfactory interpretation. The sediments of mountain border lakes also require equally detailed study for there if anywhere one should be able to find the bridge between the more distant lakes of the Great

Basin on the one hand and the mountain glaciers and Big Trees on the other.

Fortunately, in the case of the lakes of the Great Basin there is a marked recurring advance and retreat of lake waters, an exaggerated expression of climatic changes. In recent years all of the lakes, with the exception of a few of the largest, have either disappeared or almost disappeared. Their floors are now available for sectioning for the first or possibly the second time since white occupation of the West. As we have already noted, the salt content of the lakes of interior basins produces rapid deposition of even the finer sediments in contrast to the slower differential settling of fresh-water lakes. Yet the salt content was and is variable and some of the lakes now salt were once fresh through overflow at times of greater expansion, presumably in the Pleistocene though also in post-glacial time as lake has drained into lake. Moreover, it is worthwhile to look for seasonal characteristics possibly in the salt deposits themselves in the manner suggested by Gale[3] in explanation of the potash deposits of Alsace.

During 1931, 54 samples of lake sediments were collected in 13 different basins of the Great Basin province, each sample about a foot long, two inches wide and an inch deep. At the same time about 80 tree ring sections were obtained from selected localities nearby.

A comparative study is in progress to determine:

(*a*) Vertical changes in physical and chemical character.

(*b*) The degree to which the annual deposits are recognizable and vary in thickness and if such variations are cyclic or have any recognizable relation to the rainfall and evaporation records.

(*c*) The presence or absence of correspondences between the variations of thickness or constitution of lake sediments and the variation of tree ring thicknesses in sections cut from trees growing in the same watersheds.

There are at least three main lines of further investigation in the Great Basin that are held in view:

(*a*) The detailed study of *the sedimentary process* in selected sites which will show types of sedimentary alternations that may prove recognizable in sections now exposed;

(*b*) Detailed study of tree ring records on lake floors now exposed after long submergence;

(*c*) Comparative study of floor deposits in a north-south direction with the object of discovering the regional differences in sedimentation habit possibly comparable to the regional differences in rainfall habit and evaporation rate.

Both theory and fact point to the conclusion that lakes of dry regions may register the larger climatic changes even if the smaller changes go unrecorded or prove not to be recognizable. Such lakes seem to accent the degree of climatic change though with a much more pronounced lag in relation to their water-supply fluctuations in contrast to the close relation exhibited by fresh-water lakes. Those salt-water lakes that have no outlet spread widely following periods of exceptionally heavy rainfall. Following periods of drought they may dry up altogether and their total salt content is then deposited upon the floor of the basin. The recurrent and possibly periodic expansion and contraction of arid-basin lakes produces an alternation of salt and sediment that bears on its back, so to speak, the fainter alternations of the seasons and of groups of drier and wetter years. As muds settle they seal up the salts deposited in an earlier and drier phase of the climatic cycle. Only the comparative and intensive study of a widely extended series of lakes in a climatic province whose present-day or recent habits can be determined instrumentally would seem to provide an adequate observational basis for the testing of hypotheses already suggested by a number of observers.

By taking the total complexity of climate and hydrography into account we can hope to piece out the regional story which lakes seem able to tell. If their history could be stated with some degree of refinement of detail it might furnish us with the missing factors that we need to know to fill the gap between the larger and the shorter climatic oscillations. Their sediments go far back of the earliest tree records, just as the tree records go far back of the period of instrumental weather observations. Equally important is the diversity of climatic experience, region by region, in the same broad province. R. J. Russell[4] has recently represented the threefold division of the Great Basin in terms of "desert years" and Meinzer[5] has called attention to the clear distinction between the northern and southern sections of the lake region of the Great Basin in the Pleistocene as well as today. We can hardly suppose that correlation of lacustrine evidence is possible unless account be taken of regional climatic differences such as are demonstrable in the case of adjacent climatic provinces today; precipitation, for example, being in opposite phase often within relatively short distances.

The first great deficiency in correlation studies of lake sediments is the absence of detailed measurements of recent floor deposits in places where rainfall and evaporation records and tree rings are also available. Trees upon slopes adjacent to lake floors have not been systematically studied in relation to lake history. Tree trunks are known to be exposed directly upon basin lake floors that are periodically dry (yellow pine in Granite Lake, Spokane County, Washington[6]). So far as I know no one has attempted the correlation of tree ring records and sediments in critical situations nor has there yet been made a direct comparison of contemporaneous sedimentary and precipitation records.

The most neglected field appears to be essentially direct observation upon the sedimentary process. The first requirement is a technique of observation. W. A. Johnston[7] has come close to it and has shown the reality and degree of seasonal banding in the waters of a given lake, of known or knowable rainfall and snowfall and measurable stream discharge, evaporation rate, cloudiness and the like. And R. W. Sayles[8] has discussed criteria, including the important and neglected point that "the ratios between the thicknesses of the coarse and fine components should be studied rather than the actual thicknesses of the individual components, for the thicknesses of the layers are due to other factors in addition to the climatic factor." I would suggest that an analysis of the thicknesses of the winter layers might also show systematic variations of possibly diagnostic value. Bottom sampling through lake water has, when taken alone, quite limited value for the purpose in view. Deeper sections are required and an absence of marginal distortion due to the thrust of the sampling machine.

Lake sediments are, of course, in process of deposition today. Every type of lake-floor section has its counterpart in a living lake or in one but recently vanished. Moreover, we have the means available for directly observing the growth of trees in basins of sedimentation. We can directly observe the weather elements in their climatic or long-range compositions in localities of critical importance. Spotty "correlations" have high suggestive value but they do not give us laws, only miscellaneous correspondences. Certain laws of solar occurrences may be well established; the sedimentary *effects* of such occurrences are another story, a complicated story. If correspondences and resemblances are the goal, we have reached the goal. If the actual regional operation of the forces is the objective we have only opened the book. The lesson is still to be read. The status of experimental work is suggested by the fact that in his excellent paper of 1915, Gale[9] turned to the work of T. M. Chatard, published in 1890 (based on data gathered in 1886) for the composition of crystalline residues obtained by evaporating the mother liquor of a salt lake (Mono Lake, California).

The apparent conclusiveness of the first varve correlations has been misleading. Varved clays furnish a geochronological yardstick that has to be used with caution. The old question reappears: What is a correlation? There is no general agreement on the point. A wide gap has yet to be closed between the mathematics of curve analysis and the realities of the depositional process. For example, a recent paper points to solar radiation variations "not different in kind" from those of varved clays. We are not told what constitutes "difference in kind." The author is surprised to see how closely solar fluctuations "simulate" those of varved clays. What is simulation and how close must it be to be significant? A tentative correlation is offered because of "close agreement in the form of the graphs."[10] Similarities of *form* are suggestive. How can they be conclusive or become the groundwork of correlations unless independent lines of evidence give support to a theory based on mere resemblance?

The moment we detach a set of varve "curves" from the regional environment and attempt to match them with a set from a far distant locality we are bound to require a degree of correspondence in the curves that is unattainable. As Antevs[11] has pointed out: "If warm summers in one region corresponded to cold summers in another region, thick varves in the former area should be compared with thin varves in the latter. And if warm summers in one region corresponded, now with warm, now with cold summers in another region (as is the case between North America and northern Europe at present), thick varves in the former area should be compared, now with thick, now with thin varves in the latter. In other words, correlations between graphs can be made only for areas that are known, or with reasonable certainty can be assumed, to have experienced about the same annual fluctuations in the total summer temperature."

The essential point of this criticism is that connections between varve curves are not rigid and absolute but depend upon a set of independent supporting conditions of glacial or aqueo-glacial deposition. If the localities are not far apart and if the late-glacial history of the region is known in some detail, a presumption is raised or a conclusion definitely reached that the two localities had a similar climatic history during a part of the period of clay accumulation. Under these circumstances there should be possible a matching of the curves with a degree of correspondence well above the level of accident. Thus Antevs finds an equal number of curves from Brattleboro and the Hudson, 80 miles apart, in agreement to the extent of 84 per cent; Concord and Albany, 120 miles, 75 per cent; Newburg and Hartford, 70 miles, 79 percent; and so on. Upon rigid comparison, curves from the two sides of the Atlantic show correspondences in the main from 50 to 60 per cent only and thus have no meaning because "Varve graphs are individually only moderately distinctive, and, in addition, being records of summer heat, they present periodic more or less similar fluctuations."[12] Under these circumstances a close analysis of the curves and a high degree of correspondence are a rigid requirement if we are to get away from the mere opinion of an individual.

The fundamental difference between Antevs on the one hand and De Geer on the other with respect to correlations of varves as between New England and Scandinavia and Patagonia and Scandinavia is illustrated by the following quotation from Caldenius: "Two teleconnected diagrams from different territories of glaciation can hardly be expected to be similar to the same extent as two diagrams connected together from the same territory of glaciation. That would be to set expectations to too high a pitch, when we know all the matters which have a disturbing influence upon the copying of the variations of the summer temperature in the thickness of the varves. The reflecting similarities of the teleconnecting diagrams become for that reason fewer in number, and they are confined to certain groups of varves, but the teleconnections are based upon the equal length of these repeated groups of varves with their different characteristic similarities and on the equal length of groups of varves occurring between them, where conformity is missing or where the curve, on which the teleconnections are based, is indifferently formed. It is obvious that the conformity, stated in percentage indicates the similarities which are intended and are at hand, in an awkward and often misleading manner."[13]

This carries the comparison far outside the requirements of rigid correlation and into the field of simple comparison of form—and not the details of form as in the case of comparisons between varves from a given lake and its neighbor, but to comparisons of "certain groups of varves," that is, constellations of forms that simply look alike. This is suggestion, not proof; and it is valuable only if kept in the field of suggestion and not combined with sun-spot-varve correlations that have their own modes of error.

With every refinement of technique in varve studies there is an advance away from the position that varves are self-contained entities and toward the position that varves are parts of a record of which the whole is glacial history in all its complexity. One of the most puzzling features is the present-day regional variation in climatic habit. Climatic provinces—even those that are closely adjacent—do not have a uniform history. Their cycles are not commonly in the same phase if we take groups of adjacent regions into account though their phases may in time reveal causal relationships. We can only suppose that these differences have always existed. The consequence is that a single idea or a single technique of study cannot be applied wholesale. The problem requires to be broken up into its regional components.

The studies of Clayton[14] have shown what some of these regional components are. His most comprehensive generalization is that, within the tropics, the temperature increases and decreases with the solar radiation, but with a slight lag, while from 30° to 60° N. or S. there is an inversion. A correlative effect of increase of solar radiation is to intensify the belts of low pressure near the equator and near the 60th parallel and displace the intervening high-pressure belts with the consequence that there is an increase in the intensity of atmospheric circulation.

Clayton then goes on to show how this broad terrestrial generalization is affected by the well-known changes of pressure over continents and oceans with the progression of the seasons. He also carries his analysis forward to the point of including both the Arctic and Antarctic regions in the scheme of concurrent change in atmospheric pressure and solar radiation. He recognizes, however, three variables which influence "the pressure and other weather conditions at any one place on the earth surface." First is the variation of pressure intensity; second, an annual period in which pressure conditions are reversed between summer and winter, as over continents and oceans of temperature latitudes; and third, a movement of the centers of action north and south of their mean position with the varying intensities of solar radiation. He concludes, "As a result of these three variations the weather at very few places on the earth shows a simple direct influence of solar variation." By this he means that "even when the annual period is eliminated by taking the same month in succeeding years, the two remaining variables cause the weather at any given place to follow the solar radiation, sometimes directly and sometimes inversely." These complexities call for elaborate interpretations from the observations at any one station in terms of or in relation to stations elsewhere in the world. He believes that up to the present time the studies have not been conclusive and do not show the exact manner in which atmospheric waves are formed in response to changes in the atmosphere brought about by solar influence. He seems to occupy a neutral ground with respect to the changes of longer range. While believing that the day by day weather changes are directly related to solar radiation and while pointing to the close analogy between such changes and those of longer period, he concludes that the long-range changes have not yet been shown to be due to solar changes except possibly in part.[15]

In the same way that the lakes of enclosed basins exaggerate climatic changes by wide expansions and contractions, so streams exhibit cycles of run-off much more clearly than do rainfall records. This is because the run-off is cumulative and increases three times as fast as the rainfall. Correlations seem to be established between curves of stream flow, the sunspot cycle, twice that cycle, and the Brückner cycle, which is three times the sunspot cycle. The matter of stream flow has been studied in relation to the cyclical changes in lake levels, particularly the Great Lakes.[16] Thus the cumulative as well as the amplifying nature of stream flow may prove to be combinable with the records of lake-level changes and provide the sedimentation cycle of lake floors with an accented rhythm of great importance in determining the reality of solar effects. I should like to underscore this point because our attention has been focused for so long upon a special kind of lake deposit, namely, the varved clays. These were formed in glacial-marginal lakes under conditions of seasonal changes in water discharge from melting ice. They represent local drainages and special conditions of drainage that have been assumed to be exceptionally favorable to a type of lamination that records the seasons. It remains to be seen from the direct study of the sedimentation process in existing lakes of various types whether periodicities of greater amplitude may not be determinable, as for example upon lakes of high latitude or altitude whose tributary streams have strong seasonal differences of discharge and some of which are affected by ice melting and others not.

Three main lines of inquiry suggest themselves. The first is the study of the sedimentation process in lakes lying in different climatic zones; second, the study of sediments now exposed in widely contrasted climatic zones; and third, the study of changes in river discharge and lake levels in widely different climatic zones. On theoretical grounds one would conclude that the relationships between rainfall, evaporation rate and sun-spots, on the one hand, and changes in river and lake levels on the other hand, if strongly suggested or established, would vary in type and in degree of correlation as between the tropics and temperate zones and as between the humid and arid regions. Biel's recent map of rainfall variability gives us a comprehensive world view of the fact long known in meteorology that, in general, the drier the climate the greater the rainfall variability from year to year. Each type of climatic province has its special regimen of rainfall, stream discharge, change in lake level, difference in evaporation rate and in character of the sedimentary process. The result will necessarily be a marked difference in the clearness of register of these facts in the sedimentary record. It is equally certain that some types of records will necessarily show more clearly than others those relationships that are thought to be real and that as yet are expressed in correlation coefficients too low in value to be conclusive. To take a specific instance, Lake Victoria[17] in Africa, where the rainfall seems primarily responsible for variations in level of the lake, the correlation coefficient of 0.915 has been found between the rainfall of the plateau region and the level of the lake, a correlation coefficient of 0.64 being found between sun-spots and rainfall in the same region. Similarly, for Lake Nyasa, Dixey[18] has pointed out the cyclical nature of the variations in lake level and their correspondence with sun-spot numbers, the lake rising at sun-spot maxima. A much lower degree of correspondence has been observed between sun-spots and the successive levels of Lake George, Australia.[19] An 11-year periodicity seems to be recognizable and a less definite 19-year periodicity. C. E. P. Brooks in commenting upon these low correlation values observes that "weather cycles are treacherous things, and it would not be safe to base a forecast on them." His conservatism is based upon the short-range changes in Lake George. Other influences than rainfall evidently have a predominating effect and the lake may prove to be one of those having a regimen not susceptible of analysis in terms that will yield results for any of the correlations here held in view.

The phenomena that we examine in the case of climatic change come to us in diluted form. The saying is that we have to take the sunlight "as it comes to us," that is, through the atmosphere, just as we have to take the sedimentary record as it stands. And as it stands it is fragmentary, besides being the expression of a number of forces some of which are known to be without regularity of occurrence or with a regularity of such long periodicity as to have no meaning in the short-term studies with which we are here mainly concerned. Sediments, like some tree rings, may express the complex result of a number of agencies. In an attempt to correlate sediments, climatic changes, water supply, tree rings or any other set of elements, we are confronted with the difficulty of dilute effects. Advance in correlations that involve sediments seems to depend upon a new technique of study of the sedimentation process and the search for lakes, whether existing or extinct, whose bottom deposits have been in delicate relation to specific climatic conditions. The lakes of the Great Basin are among those which seem to promise data of critical value.

[1] Chester A. Reeds, "Weather and Glaciation," *Bull. Geol. Soc. Am.*, **40**, 597–629 (1929).

[2] Ernst Antevs, "On the Pleistocene History of the Great Basin," in *Quaternary Climates*, Carnegie Institution Publication 352, *Washington*, 66 (1925).

[3] Hoyt S. Gale, "The Potash Deposits of Alsace," *Bull.* **715-B**, *U. S. G. S.*, p. 48 (1920): " . . . the chill of winter would precipitate sylvite in a thin layer, and the evaporation of the following summer would precipitate a corresponding layer of common salt. The process evidently went on for 25 to 50 years or more, until a considerable deposit of the mixture of these two salts, known as sylvinite, had accumulated."

[4] R. J. Russell, "Dry Climates of the United States. II. Frequency of Dry and Desert Years, 1901–20," *University of California Publications in Geography*, **5**, No. 5 (1932).

[5] Oscar E. Meinzer, "Map of the Pleistocene Lakes of the Basin-and-Range Province and Its Significance," *Bull. Geol. Soc. Am.*, **33**, 541–552 (1923).

[6] Otis W. Freeman, "Evidence of Prolonged Droughts on the Columbia Plateau Prior to White Settlement," *Monthly Weather Review*, **57**, 250–251 (1929).

[7] *Am. Journ. of Sc.*, **4**, 5th Ser., 376 (1922).

[8] Memoirs, *Mus. Comp. Zoöl.*, **47**, No. 1, 34 (1919).

[9] Hoyt S. Gale, "Salines in the Owens, Searles and Panamint Basins, Southeastern California," *Bull.* **580**, *U. S. Geological Survey*, 251–323 (1915).

[10] Chester A. Reeds, "Weather and Glaciation," *Bull Geol. Soc. Am.*, **40**, 597–629 (1929). Reference on pp. 625–626.

[11] Ernst Antevs, "Late-Glacial Correlations and Ice Recession in Manitoba," *Memoir* **168**, *Geological Survey of Canada*, 35 (1931).

[12] Ernst Antevs, *Ibid.*, 36.

[13] Carl C:zon Caldenius, "Las Glaciaciones Cuaternarias en la Patagonia y Tierra del Fuego," *Fr. Stockholms Högskolas Geokronol. Inst.*, *Data*, **17** (*Geografiska Annaler*, 1932, H. 1 o. 2), 154–155.

[14] H. H. Clayton, *World Weather*, New York, 221–222 (1923).

[15] H. H. Clayton, Op. cit., p. 283.

[16] A. Streiff, "The Practical Importance of Climatic Cycles in Engineering," *Monthly Weather Rev.*, **57**, 405–411 (1929).

[17] C. E. P. Brooks, "The Fluctuations of Lake Victoria," *Jour. East Africa and Uganda Nat. Hist. Soc.*, 47–55 (June, 1925).

[18] F. Dixey, "The Physiography of the Shire Valley, Nyasaland, and Its Relation to Soils, Water Supply and Transport Routes," *Geological Survey Department* (1925).

[19] C. E. P. Brooks, "Variations in the Level of Lake George, Australia," *Nature*, **112**, p. 918 (1923).

In response to the request of the Chairman for discussion of the above contributed papers, the following remarks were made from the floor by Messrs. A. E. Kennelly, E. W. Brown, H. N. Russell and E. B. Wilson.

We seem to be justified in concluding from the interesting papers, just read, that correlations are found between variations in certain solar phenomena (sun-spots and radiation) and variations in certain terrestrial phenomena (magnetic, thermal and vegetative). These correlations are not sufficiently clear to warrant definite conclusions at present; nevertheless, they seem to be sufficiently evident to make more extended studies well worthwhile, especially when we consider how recent are the observations on which these correlations have been obtained. Quantitative sun-spot records are not much more than one century old, and quantitative radiation records only a few decades. These intervals are but droplets in the stream of solar and terrestrial time.

It would be most interesting to unravel, if possible, the history and causes of the sun-spot cycles. How far back can they be traced in vegetation? Are they discernible in the sections of fossil trees? Are these spot cycles caused by actions below the solar surface, or in external space? These cosmic phenomena are surely eminently worthy of study by the National Academy of Sciences. Even if we assume that their complexity will baffle all attempts at future long-range weather forecasting on the earth, they may be expected to lead to knowledge of the past history of the earth and sun. We are, therefore, justified in believing that further research will establish historical connections between the sciences of geology, heliology and meteorology.

A. E. KENNELLY

The analysis of a continuous series of observations into cycles, usually called harmonic analysis, is useful:

(*a*) When physical theory indicates the presence of a cycle and we wish to know its amplitude and phase or to correct its period;

(*b*) When it is desired to discover a cycle either for purposes of prediction or to give an indication of physical theory.

Experience has, however, shown that the discovery of a cycle which was not obvious from the graph has little value. A *hidden* cycle means that it is so overlaid with other effects that it cannot be used for prediction or for the discovery of the principal physical causes of the oscillating phenomena.

Analysis into *many* hidden cycles has usually no physical meaning. Mathematical theory shows that we can always represent a graph by cycles. Since each harmonic term introduces three constants, the more cycles we take the better should be the representation.

A partial test of the reality of a cycle is furnished by calculation of the probable errors of its constants. This should always be done whether the cycle is hidden or not.

The question of correlation between two sets of oscillating phenomena is to some extent mixed up with that of harmonic analysis. The mere fact that the two sets appear to give a cycle which has nearly the same period in each has usually no physical meaning outside of a known physical cause. Where several cycles are found, the laws of chance alone may give us one in one set which nearly corresponds with one in the other set. This is particularly the case with the sub-harmonics. If we find a cycle for example, of twelve years, we are almost sure to find one or more of six, four, three, etc., years. If we find only an occasional multiple, the cycle should be regarded as highly suspicious as to its reality.

The question as to the degree of correlation between two sets of phenomena needed to assert the existence of a common physical connection, is to some extent an open one. Usually, however, unless a correlation is obvious it is scarcely worth detailed study for the same reasons as those given for the existence of a cycle.

I am in full agreement with Humphreys when he states that there are only two known climatic cycles, the day and the year and these result from geometrical changes of position of the earth's surface with regard to the sun. The nature of the motion of a fluid like the atmosphere is such that irregularity is almost the necessary consequence—that is, that it is impossible to predict its motions by mathematical cycles alone. The more promising line of attack is that which has already been successful, namely, correlation of phenomena which follow one another in time though not necessarily in place. The physical basis for this is the fact that the motions of the atmosphere and their effects depend to a greater extent on phenomena which have immediately preceded them than on those which are separated from them by a longer interval of time.

"Trigger" action is sometimes invoked. Such action demands a peculiar set of circumstances which will only rarely occur and it involves the existence of instability. When instability is present predictions as to the time when future events will take place are rarely possible.

E. W. BROWN

The conspicuous periodicities which appear in solar phenomena (and the more doubtful ones which are found in the weather) differ radically from those familiar to the astronomer. The latter depend, in one way or another, on simple dynamical motions (such as the Earth's rotation and orbital revolution) and are precise timekeepers. A similar oscillation in a body of the Sun's mass and density should have a period of two hours or less. The sun-spot cycle is more than fifty thousand times longer, and is evidently based on some fluctuation of thermal or hydrodynamic character. Sun-spots are periodic in space (heliographic latitude) as well as in time; and this, and the reversal of polarities in successive cycles, shows clearly that each cycle is a unit—an outburst which affords a release for some sort of internal strain, each beginning before the last died out. One would not expect such cycles to be more than very roughly periodic, like the eruptions of one of the more regular geysers.

This makes an analytical treatment of the periodicity very difficult, for almost all the available methods assume the existence of fixed underlying periods. The formulae derived from such analyses habitually fail to predict the future course of the phenomena, not on account of any errors in the reckoning, but because the processes themselves are not accurately predictable. Michelson, whose judgment in physical matters was excellent, after studying the sun-spot curve, concluded that it should be represented by a function of variable period, amplitude and phase.[1]

In the case of the weather, we have conspicuous, but not exactly repeating, cycles of diurnal and annual period, weak, but probably real, evidences of the main solar cycle, and evidence also of other cycles, which may be determined by terrestrial conditions, for example, in the atmosphere or the ocean. These, like the solar cycle, are probably inherently irregular in period and amplitude. Superposed on them are the much larger variations connected, for example, with cyclonic storms.

To disentangle the various cycles from one another and from the other fluctuations, is a task of excessive difficulty, and I fear that even generalized prediction of weather conditions by the extrapolation of empirical cycles derived from observation, if attainable, is far in the future.

This opinion, though unfavorable to certain methods of prediction, is very far from

[1] *Astrophys. Jour.*, **28**, 273 (1913).

being adverse to the continuance of such admirable investigations as Dr. Abbot and Dr. Douglass have described. Each of these researches has already far more than justified its existence and its continuance. The one has provided a precise chronology of fundamental value to the western archaeologist; the other has proved the existence of fluctuations in the sun's radiation, enduring for months or years, whose explanation offers important problems which the astrophysicist is as yet hardly ready even to attack. Further investigation along these and similar lines is sure to be fruitful and may lead to important practical results.

H. N. RUSSELL

Previous speakers have referred to empirical statistical analysis on the one hand and to analytical mathematical analysis on the other. I wish to mention a point which is of equal importance to both. I have worked very little with climatic cycles. The only work of that sort which I have done is in attempting to find some reasonably high correlations between the death rate from penumonia and various aspects of the weather such as temperature, the diurnal variation of temperature, or the suddenness of temperature changes, or humidity or barometric pressure, and I have found that the correlations are low. Even if one treats the problem by partial correlation one has an outstanding variation which is large.

Important as such a treatment is whether for the empirical descriptive representation of a part of the phenomenon or as a suggestion for an analytical and mathematical discussion of the rational basis of the phenomenon, it is scientifically of the utmost importance not to give up the exploration for other possible variables which may be much more highly correlated with the phenomenon. It is quite possible that in respect to the mortality from respiratory diseases or the growth of trees some variable not now tabulated in the meteorological records is of more crucial importance than any of the variables which are tabulated. For example, there is the ionization of the air, which is decidedly variable, on which certain preliminary experiments and calculations (which are not ready for release because the series is still too short to be dependable) indicate the possibility that it may be even more highly correlated with the respiratory mortality than any other variable yet tried. The search for new and better variables is fundamental to both the empirical and the analytical aspects of science.

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